EXAMINATION *of the* SUPPOSED IGNEOUS ORIGIN *of* STONY SUBSTANCES. *By* RICHARD KIRWAN, *Eſq; F. R. S. and M. R I. A.*

Read Feb. 3, 1793.

THE origin of ſtony ſubſtances, ſeemingly an object of mere curioſity, is nevertheleſs connected with deſiderata of the greateſt utility to mankind; to ſay nothing of the arts of fabricating glaſs, artificial gems, mortar, cement, bricks, pouzzolane and earthen-ware, which have evidently ſome reference thereto, nature preſents us with various ſtones, as ſlates, mica, &c. with whoſe artificial compoſition, though highly important, we are at preſent unacquainted, and muſt remain ſo until the mode of their production is ſatisfactorily aſcertained; then, and then only, experiments tending to form them by art may be planned and attempted. Hence the propriety of examining the foundation of the different opinions of mineralogiſts and geologiſts on this head; if we can exclude any of them, we are ſo much the

nearer to the proper end of our enquiries. Fortunately in this caſe, relatively to the general mode, there neither are nor can be more than two opinions; if one can be proved falſe, the other muſt of neceſſity be true. All are agreed that ſtony ſubſtances were originally ſoft, and even liquid; but ſome think this fluidity was occaſioned by their having been diſſolved, or at leaſt diffuſed in water, and afterwards cryſtallized, precipitated, or otherwiſe ſeparated from it. This is, at this day, the doctrine moſt generally adopted by mineralogiſts; volcanic ſtones, by far the leaſt numerous of the ſtony tribe, forming but a trifling exception to this general fact.

LATELY, however, a very different theory has been offered to the public in the firſt volume of the Tranſactions of Edinburgh, and ably ſupported by Doctor Hutton, a gentleman advantageouſly known to the philoſophic world through an important meterological diſcovery, the General Law of the Formation of Rain. His opinion ſeemingly reſembles, but in fact differs eſſentially, from thoſe entertained by Leibnitz, Telliamed, Moro and Buffon. They held ſtones to have been originally ſolid, and afterwards liquified by heat; but Doctor Hutton endeavours to prove that previous to the preſent ſtate of our globe they were utterly deprived of ſolidity, and have ſince acquired it by fuſion, and ſubſequent congelation on cooling. This ſyſtem, the moſt ingenious certainly that has as yet been deviſed, is however but little known or at leaſt noticed on the continent; the few that have mentioned it do not ſeem to have well comprehended it; in thoſe points of view in which I have

I have conſidered it, I muſt ſay it appears to me improbable; upon diſcuſſion, however, it is poſſible my objections may be removed; thoſe that have occurred to me I ſhall now briefly ſtate to the Academy in the cleareſt and ſhorteſt manner I am able.

AFTER ſome profound reflections on the wiſdom diſplayed in the conſtitution of the globe we inhabit, our author tells us, "It is neceſſary to diſtinguiſh three different bodies which "compoſe the whole of it; a ſolid body of earth, an aqueous "body of ſea, and an elaſtic fluid of air. There is a central "body in the globe which is commonly ſuppoſed to be ſolid "and inert, but which he will afterwards prove not to be ſo. "There is alſo an irregular body of land raiſed above the level "of the ocean, which is doubtleſs the ſmalleſt portion of the "globe. There is alſo an atmoſphere of air, neceſſary for the "ſuſtenance of fire, animal life, vegetation," &c. *Pages* 211 *and* 212.

AFTER mentioning the general powers that actuate the whole machine, he confines his views to that part which we inhabit. "That we may conſider the *natural conſequences* of thoſe opera- "tions, which, being within our view, we are better qualified "to examine. In purſuit of this object we employ our ſkill "*in reſearch*, and not in forming *vain conjectures*, and, *if data* "*are to be found*, on which ſcience may reaſon, we ſhould "not long remain ignorant of the natural hiſtory of the globe, "a ſubject on which opinion only and not evidence has "hitherto

" hitherto decided; for in this ſubject there is naturally leſs " defect of evidence, although philoſophers, led by prejudice or " miſguided by falſe theory, have neglected to employ that " light." This paragraph ſeems to me ſomewhat obſcure; the exiſtence of thoſe data on which ſcience is to reaſon being only hypothetically laid down in the firſt part, but in the concluſion the exiſtence of ſuch data ſeems poſitively affirmed, ſince philoſophers are reproached with neglecting to employ the light reſulting from them; this is however but of little conſequence. " But to proceed, in purſuing further our general preparatory " ideas, a ſolid body of land could not anſwer the purpoſe of " an habitable world, for a *ſoil* is neceſſary to the growth of " plants, and this conſiſts of materials collected from the " deſtruction of ſolid land Therefore the ſurface of the land " inhabited by man is made by nature to *decay*, in diſſolving " from that hard and compact ſtate in which it is found below " the ſoil, and this ſoil is neceſſarily waſhed away by the con- " tinual circulation of the water running from the ſummits of " the mountains towards the general receptacle of that fluid." *Page* 214. Here we muſt arreſt the courſe of our ingenious philoſopher. He aſſerts that the terreſtrial part of the globe was originally a ſolid compact maſs, from the diſſolution of which the leſs compact and looſer earths, as chalk, clay, magneſia and moulds have ariſen. This preliminary propoſition cannot be allowed; for the earthy and ſtony part may be of equal antiquity, or the terreſtrial part may have been originally formed in a highly comminuted ſtate, and have afterwards partially coaleſced into ſtony maſſes, and the remainder may have

have continued in its original ſtate, or nearly ſo, its particles having acquired only that degree of conſiſtence which we obſerve in clays and earths. That the ſoil, however, receives an increaſe from ſome ſpecies of ſtones that moulder by expoſure to the air cannot be denied, but there is no proof that *all ſoil* has ariſen from decompoſition. He next tells us that this ſoil is neceſſarily waſhed away by the continual circulation of water running from mountains to the ſea. Here are two ſuppoſitions, neither of which is grounded on facts. Soil is not conſtantly carried away by the water, even from mountains, as Mr. De Luc has clearly ſhewn in his nineteenth and twentieth letters to the Queen*; and if it were, it would be depoſited on the plains, for there are plains as well as mountains on the dry parts of our globe. All water does not flow into the ſea; much of it is carried off by evaporation. Moſt of the earth ſwept off by rivers is depoſited at their mouths; of that which is carried into the ſea, much, if not all, is rejected on the ſhore. Neither has the ſea that deſtructive action on the ſhores univerſally that Buffon and others have ſuppoſed. This is evident by inſpecting the baſaltic pillars on the coaſt of Antrim; the angles of ſuch of theſe as are and have been expoſed to the waves, perhaps for ſome thouſand years, are juſt as ſharp as thoſe of ſuch pillars as are placed far beyond their reach.

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* Theſe letters and ſeveral other papers of this excellent philoſopher in Rozier's Journal contain much uſeful information on geological ſubjects. But unhappily it muſt be purchaſed by a great expence of *time*.

HENCE the concluſions of our author relative to the imperfect conſtitution of the globe fall to the ground; and the pains he takes to learn "by what means a decayed world may be "renovated," are ſuperfluous. "But further (continues our "author) the ſolid parts of the globe are in general compoſed "of ſand, gravel, argillaceous and calcareous ſtrata, or of "various compoſitions of theſe with other ſubſtances." This certainly cannot be aſſumed as a fact, but rather the contrary; it holds true only of the ſurface, the baſis of the greater part of Scotland is evidently a granitic rock, to ſay nothing of the Continents both of the Old and New World, according to the teſtimony of all mineralogiſts.

IN the ſucceeding paragraphs, pages 119 and 120, there is ſome ambiguity, which it is proper to explain. In all regions of the globe immenſe maſſes are found, which though at preſent in the moſt ſolid ſtate, appear to have been formed by the collection of the calcareous *exuviæ* of marine animals. "That "all maſſes of marble or limeſtone are compoſed of the calca-"reous matter of *marine bodies* may be concluded from the "following facts:—Firſt, few beds or marble or limeſtone occur "in which may not be found ſome of thoſe objects which "indicate the marine origin of the maſs. We ſhall thus find "the greater part of the calcareous maſſes upon the globe to "have originated from *marine calcareous bodies*. That theſe beds "had their origin at the bottom of the ſea, and that they have "the calcareous ſubſtance which they contain, from the ſame "ſource as marble or limeſtones." If by *marine origin* the author

author means that moſt or all calcareous mountains were formed in the ſea, this will not be diſputed; but if his meaning be, that all calcareous matter conſiſts of marine exuviæ, this cannot be allowed, as huge maſſes of marble exiſt which diſcover not the leaſt trace of marine exuvia, and calcareous ſubſtance is found in many granites and the component parts of granite, which was never ſuſpected to be of teſtaceous origin. The exiſtence of ſuch maſſes is not diſputed by our author. "There are "(ſays he) in all regions of the earth, huge maſſes of calcareous "matter, in a cryſtalline or ſparry ſtate, in which, perhaps, no "veſtige can be found of any organized body, nor any indication "that ſuch calcareous matter had belonged to any animals, but "as in other maſſes this ſparry ſtructure or cryſtalline ſtate is "evidently aſſumed by the marine calcareous ſubſtances in oper-"ations which are neceſſary to the conſolidation of the ſtrata, it "does not appear that the ſparry maſſes in which no figured body "is formed have been originally different from other maſſes "which leave ample evidence of their marine origin." That is to ſay, ſince ſparry maſſes are found among calcareous ſtrata of teſtaceous origin, other ſparry maſſes may alſo have the ſame origin. This reaſoning does not appear to me at all concluſive, any more than if an inhabitant of the interior parts of a continent, unacquainted with any calcareous ſtones but thoſe of a ſparry ſtructure, ſhould conclude that all this matter originally proceeded from the bones of land animals, becauſe they alſo are of a calcareous nature. It is much more probable that ſea animals themſelves derive their calcareous matter from a pre-exiſting ſubſtance of the ſame nature contained in their food, as we have no proof of the actual

productibility of any ſimple earth. Our author's concluſion, however, is, " That all the ſtrata of the earth, not only thoſe conſiſt-
" ing of calcareous maſſes, but others ſuperincumbent on theſe,
" have had their origin at the bottom of the ſea, by the collection
" of ſand, gravel, ſhells, coralline and cruſtaceous bodies, and of
" earths and clays variouſly mixed, ſeparated and accumulated," page 221. Various geological obſervations contradict this concluſion. There are many ſtratified mountains of argillaceous ſlate, gneiſs, ſerpentine, jaſper, and even marble, in which either ſand, gravel, ſhells, coralline, or cruſtaceous bodies are never or ſcarce ever found *.

THE general amount of our author's reaſoning however is, " That nine-tenths perhaps, or ninety nine hundredths of this
" earth, ſo far as we ſee, have been formed by natural opera-
" tions of the globe, in collecting looſe materials and depoſiting
" them at the bottom of the ſea; conſolidating thoſe collections
" in various degrees, and either elevating thoſe conſolidated
" maſſes above the level on which they are formed, or lowering
" the level of the ſea." How ill ſupported by facts this concluſion is we have already ſhewn; and our author himſelf will preſently diſcover, for he adds, " There is a part of the
" ſolid earth, which we may at preſent neglect, not as being
" perſuaded that this part may not alſo be found to come under
" the general rule of formation with the reſt, but as conſidering
" it as of no conſequence in forming a general theory which
" ſhall

* 1 Gerh. Geſch. p. 72. 85. 2 Gerh. 413.

" ſhall comprehend almoſt the whole." This excluded part conſiſts of mountains and maſſes of granite. And yet moſt geologiſts look on this excluded ſubſtance as forming by far the greater part of the globe, all other parts being commonly found to reſt upon it *.

HAVING thus found the greater part, if not the whole of the ſolid land, to have been originally compoſed at the bottom of the ſea, our author proceeds to examine, how ſuch continents as we now have could be erected above its level; he ſhews that no motion of the ſea could produce that effect; or if it could, yet ſuch a continent could not produce maſſes of ſolid marble and other minerals in a ſtate very different from that in which they were originally collected. " Conſequently, beſides an operation " by which the earth at the bottom of the ſea ſhould be con- " verted into elevated land, a *conſolidating* power is required, " by which the looſe materials ſhould be formed into maſſes of " the moſt perfect ſolidity; and, if this were underſtood, " we might poſſibly become acquainted with the power that " elevated our continents above the level of the waters." Of this conſolidating power he treats in the ſecond part of his eſſay.

BEGINNING his ſecond part, he reaſons thus, p. 225. " There are juſt two ways by which porous and ſpongy bodies " may be conſolidated into maſſes of a natural ſhape and

 " regular

* Hoffm. in Berg. Kalend. 197. Voight. 7. Gerhard, Bergman, Pallas, &c.

" regular ſtructure; the one congealation from a fluid ſtate by " means of *cold*; the other *accretion*, and this includes a ſepa- " ratory operation; to produce ſolidity either way, fluidity muſt " be induced, either by heat or by a ſolvent." This reaſoning tacitly ſuppoſes a fact which we have already ſeen to be either falſe or precarious. The particles which now form the ſolid parts of the globe need not be ſuppoſed to have originally been either ſpungy or porous, the interior parts at the depth of a few miles might have been originally, as at preſent, a ſolid maſs. The more ſuperficial maſſes might have been partly diffuſed and partly diſſolved in the primogenial fluid. The particles, for inſtance, of which argillaceous ſlates were formed, might have been originally barely diffuſed, as they ſeem to have been formed by mere ſubſidence. " The ſtrata formed at the bottom " of the ſea are to be conſidered as having been conſolidated " either by aqueous ſolution and cryſtallization, or by the " effect of heat and fuſion; if by the firſt of theſe two ways " the ſolid ſtrata have attained their preſent ſtate, there will be " a certain uniformity obſervable in the effects; and general " laws by which this operation muſt have been conducted." Here ſubſidence and precipitation, as part of the general means of the formation of ſtones in the moiſt way, ſhould not have been omitted.

As to the uniformity to be expected in the effects of cryſtallization, the learned author is certainly too well acquainted with the ſubject not to know that this uniformity is not to be expected but when all the circumſtances are perfectly ſimilar.

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He muſt know that ſuperſaturation, a ſlight contamination with certain heterogeneous ſubſtances, a variation in the temperature, a variable degree of agitation, a difference in the quantity of the menſtruum, or in the time of its diſparition, beſides many other unknown circumſtances, daily produce different effects in the cryſtallization of ſalts, the bodies in which this operation has been moſt attended to.

" But water (continues he) being the general medium in which " bodies collected at the bottom of the ſea are always con- " tained, if thoſe maſſes of collected matter are to be conſolidated " by ſolution, it muſt be by the diſſolution of thoſe bodies " in that water as a menſtruum, and by the concretion or " cryſtallization of that diſſolved matter, that the ſpaces firſt " occupied by water in thoſe maſſes are afterwards to be filled " with a hard and ſolid ſubſtance ; but without ſome other power " by which the water contained in thoſe cavities and endleſs " labyrinths of the ſtrata ſhould be ſeparated in proportion " as it has performed its taſk, it is inconceivable how thoſe " maſſes, however changed from the ſtate of their firſt ſub- " ſidence, ſhould be abſolutely conſolidated, without a particle " of fluid water in their compoſition." Abſtracting from his own gratuitous hypotheſis, it is very eaſy to ſatisfy our author on this head; the concreting and conſolidating power in moſt caſes ariſes from the mutual attraction of the component particles of ſtones to each other; if theſe particles leave any interſtices, theſe are filled with water which no way obſtructs their ſolidity when the points of contact are numerous; hence the decrepitation of

of many ſpecies of ſtones when heated. Many ſorts of ſtones are ſoft while in their ſtrata, as ſandſtones, limeſtones, &c. but loſe their water and acquire hardneſs by expoſure to the air. But perhaps the moſt effectual means of convincing our author that a conſolidating power *may* take place in water, is to remind him that in many caſes it does *actually* take place. Thus mortar made by pouzzolona or terras, is well known to harden under water; nay Mr. Smeaton has obſerved it to throw out under water a ſtalactite, which alſo hardens in that ſituation. *Eddy ſtone*, § 181. The calcareous depoſits formed in certain waters, and which attain a ſtony hardneſs, are a further proof that immerſion in water does not always obſtruct the formation of ſolid maſſes. " But (adds our author) we " find ſtrata conſolidated by various ſubſtances which water " cannot diſſolve; thus we have water conſolidated by calcareous " ſpar, a thing perfectly diſtinguiſhable from the ſtalactitical " concretion of calcareous earth, in conſequence of aqueous " ſolution; we have ſtrata made ſolid by the formation of " fluor, a ſubſtance not ſoluble, as far as we know, by water; " we have ſtrata conſolidated by ſiliceous earth in a ſtate wholly " different from that in which it was obſerved, on certain " occaſions, to have been depoſited by water; we have ſtrata " conſolidated by ſulphureous and bituminous ſubſtances which " refuſe a watery ſolution; laſtly, we have ſome conſolidated " by almoſt all the various metallic ſubſtances. If it is by " means of water that all theſe interſtices have been filled " with thoſe materials, water muſt be, like fire, an univerſal " ſolvent,

" ſolvent, and we muſt change our opinion of its chymical " character."

HERE the difficulties to the ſuppoſition of an aqueous ſolution are placed in the ſtrongeſt light; yet it muſt be owned that they partly ariſe from the author's own gratuitous ſuppoſition, that ſtrata exiſted at the bottom of the ſea previous to their conſolidation; a circumſtance which will not be allowed by the patrons of the aqueous origin of ſtony ſubſtances, as we have already ſeen.

SECONDLY, That water, in certain circumſtances, and with the addition of certain ſubſtances, may be admitted as a univerſal ſolvent, ſhould not be denied, merely on account of our ignorance of thoſe circumſtances and auxiliary ſubſtances. Before the diſcovery of the ſparry acid, it was not known that water, by the aid of that acid, could diſſolve ſiliceous bodies, a power which, by this help, it is now known to poſſeſs; there may be various other menſtrua in nature of which we are as yet ignorant; it is well known that certain proportions of the ſimple earths act upon each other as menſtrua in the dry way, why not alſo in the moiſt way, if equally divided? and what hinders us from ſuppoſing that they were originally created in that ſtate of diviſion that would render them capable of acting on each other? why ſhould we ſuppoſe this habitable earth to ariſe from the ruins of another anterior to it, contrary to reaſon and the tenor of the moſaic hiſtory? What do we gain by that ſuppoſition? Muſt not the origin

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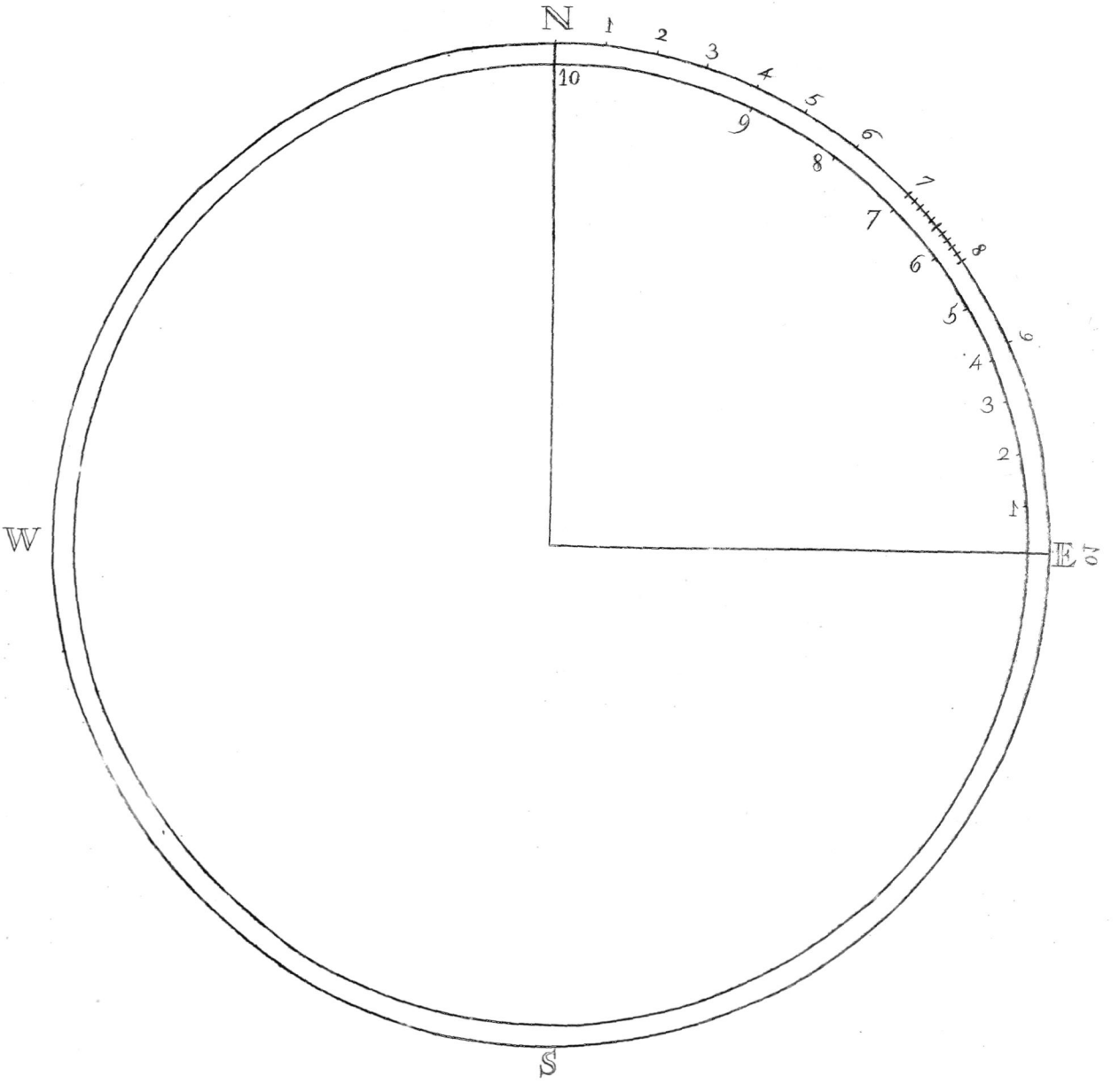
of that anterior world, if compoſed of materials ſimilar to thoſe of this, be equally accounted for? and muſt we ſuppoſe that anterior world deſtitute of calcareous earth becauſe it was not formed at the bottom of the ſea? If it were deſtitute of that earth, it could not contain plants or animals ſimilar to ours, as ours eſſentially require that earth: or muſt we allow that anterior ſolid land to have been itſelf alſo formed of the ruins of another ſtill prior to it, and thus admit a proceſs *in infinitum*; an abyſs from which human reaſon recoils? Into this gulph our author however boldly plunges; towards the end of his Eſſay he tells us, this earth is derived partly from one immediately anterior, and partly from another anterior to that again. In a word, to make uſe of his own expreſſion, "We find no veſtige of a beginning." Then this ſyſtem of ſucceſſive worlds muſt have been eternal; now ſucceſſion without a beginning is generally allowed to involve a contradiction, therefore the ſyſtem that forces us to adopt that concluſion muſt neceſſarily be falſe. Our author was led to it by his, and our common ignorance, of the means by which ſtones of the ſiliceous claſs were conſolidated or diſſolved in liquid menſtruums, but the rules of exact reaſoning require that, before we deny the general poſſibility of producing an effect by any given cauſe, we ſhould be acquainted with all the poſſible methods of applying that cauſe; if any of them be unknown, our concluſion muſt be defective; more eſpecially if we have ſtrong reaſons to ſuſpect that ſome modes or circumſtances in the application, that cauſe do exiſt with whoſe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whoſe detail we are unacquainted. Now this happens to be the caſe with reſpect to the ſolution of earths or ſtones of the ſiliceous kind. Mr. Bergman had already obſerved that ſiliceous earth, ſufficiently divided, was ſoluble in all acids *. Mr. Klaproth, the worthy ſucceſſor of the immortal Scheele, found it ſoluble in mere water in the ſame ſtate of diviſion †. The great geologiſt, Mr. Dolomieu, ſeems to have diſcovered, by the help of the chymical abilities of Mr. Pelletier, the very circumſtances on which its ſolubility depends. Mr. Morveau has alſo diſcovered another, and a very different method of effecting this ſolution ‡. Mr. Laſſone found the ſurface of grit, which had been broken a year before, inveſted with ſiliceous cruſt nearly as hard as agate, which therefore muſt have been newly formed. Hence ſtrata might be conſolidated without fuſion §. It were eaſy, but needleſs, to accumulate more teſtimonies of this fact, as other proofs of ſuch production will occur in the ſequel. Dolomieu obſerved the growth of ſhorls on the Pyrenees ‖.

Our author next proceeds to ſtate, " That if it is by means " of heat and fuſion that the looſe and porous ſtructure of the " ſtrata ſhall be ſuppoſed to have been conſolidated, then " every difficulty which had occurred on the power and agency

* 5 Bergm. 128, in Note.
† 3 Berl. Beobacht. 160.
‡ Swed. Abhandl. 1790.
§ Mem. Paris, 1774. 13 in 8vo.
‖ Surles Iſles Pouces. 249.

" of water is at once removed; the looſe and diſcontinuous body " of a ſtratum may be cloſed by means of *ſoftneſs and com-* " *preſſion*, the porous ſtructure of the materials may be con- " ſolidated in a ſimilar manner by the *fuſion* of their ſub- " ſtance, and foreign matter may be introduced into the open " ſtructure of ſtrata, in form of *ſteam or exhalation*; conſequently " heat is an agent competent for the conſolidation of ſtrata, " which water alone is not. The examination of nature gives " countenance to this ſuppoſition; conſequently, however difficult " it may appear to have this application of heat, we cannot " from natural appearances ſuppoſe any other cauſe." *Pages* 229, 230, and 237. He adds " For the explanation of thoſe natural " appearances which are ſo general, no further conditions are " required, than the ſuppoſition of a ſufficient intenſity of fire or " heat, and a ſufficient degree of compreſſion upon thoſe bodies, " which are to be ſubjects to that violent heat, without " calcination or change. So far as this ſuppoſition is not gratuitous, " the appearances of nature will be thus explained."

HERE we have the whole theory of our author; in oppoſition to which I make bold to ſay, 1ſt, That the ſuppoſition of a degree of heat under any given compreſſion, ſufficient for the fuſion of ſtony ſubſtances in general, without calcination or change, is not only gratuitous, but contrary to all that we at preſent know of the agency of heat. Secondly, That all the appearances of nature depoſe in favour of an aqueous ſolution or diffuſion, and a cryſtallization, concretion or ſubſidence therefrom, and againſt an igneous ſolution or fuſion.

AND

And firſt, it is *gratuitous*, not only becauſe it is unneceſſary, as we have already ſhewn, but alſo becauſe it is inconſiſtent with our author's own theory. According to him theſe ſtrata which were conſolidated by heat were compoſed of materials gradually worn from a preceding continent, caſually and ſucceſſively depoſited in the ſea; where then will he find, and how will he ſuppoſe, to have been formed thoſe enormous maſſes of ſulphur, coal, or bitumen neceſſary to produce that immenſe heat neceſſary for the fuſion of thoſe vaſt mountains of ſtone now exiſting? All the coal, ſulphur, and bitumen, now known, does not form the $\frac{1}{100000}$ part of the materials depoſited within one quarter of a mile under the ſurface of the earth; if therefore they were, as his hypotheſis demands, carried off and mixed with the other materials, and not formed in vaſt and ſeparate collections, they could never occaſion, by their combuſtion, a heat capable of producing the ſmalleſt effect, much leſs thoſe gigantic effects which he requires. Again, it *is contrary to all we know of the action* of heat; by this we are informed that heat may be produced among *hard* bodies by attrition, and in *inflammable* bodies by combuſtion. To produce heat by attrition it is neceſſary that the bodies rubbed together be ſo hard as that their particles ſhould not eaſily be abraded, and alſo that they be perfectly dry; if, therefore, the ſtrata formed in the bed of the ocean were looſe, porous, and ſpongy, previous to the production of heat, and alſo intimately penetrated with water, as our author repeatedly aſſerts, it is evident, from all we at preſent know, that no degree of attrition which they might endure could produce the ſmalleſt

degree of heat. Even if it could produce ſome heat, nay an intenſe heat, yet a heat productive of fluidity could never be the conſequence of attrition, for the inſtant the bodies ſubjected to it would acquire the firſt degree of emolleſcence, the calorific power of attrition muſt neceſſarily ceaſe.

But granting to our author (what we have ſhewn to be inconſiſtent with his theory) ſuch immenſe maſſes of coal, ſulphur, and bitumen as muſt be ſuppoſed collected together, to procure by their inflammation a heat of energy ſufficient to melt all the ſtony ſubſtances now exiſting, I proceed to ſhew, that, conſiſtently with the laws of nature hitherto known to us, either no inflammation at all could be produced, or at leaſt none capable of producing the effects required by him. To produce inflammation the preſence of vital air is neceſſary; to produce an immenſe inflammation of energy ſufficient to melt ſtony ſubſtances, not only an immenſe collection of ſuch air, but of air of the greateſt purity, is required. To produce an inflammation, capable, if poſſible, of melting ſtony ſubſtances without effecting either calcination or any other change in them, an immenſe compreſſion muſt alſo be ſuppoſed. Now, granting to our author a collection of coal, bitumen, &c. ſufficient to produce a flame capable of ſuch mighty effects, where ſhall we find air to ſupport that flame? neither coal nor bitumen produce vital air. But ſuppoſe this coal or bitumen mixed with ſubſtances capable of giving out that air, ſtill that air would be ſo impure, from the mixture of the fixed and mephitic airs ariſing from the coal or bitumen, as to be

be incapable of forming a heat even equal to that of our common furnaces, as Mr. Dolomieu has clearly ſhewn to be the caſe with reſpect to volcanic heat; and again, allowing our author air of ſufficient purity to effect his purpoſes, what ſhall prevent the water, ſuperincumbent on his looſe, ſpongy ſtrata, and preſſing upon them with immeaſurable weight, from penetrating through theſe ſtrata, and extinguiſhing this flame, or at leaſt from preſſing down the incombuſtible ſtrata upon the inflamed coal, and, by intercepting all communication with the air, from immediately ſuppreſſing the flame? What ſhall prevent the air itſelf, rarefied by heat, from eſcaping through the looſe, and as yet, unconſolidated ſtrata? But to favour the author ſtill further, let us ſuppoſe the fire to originate in caverns formed within the coaly matter itſelf, and let us ſuppoſe ſuch vaults (contrary to all probability) capable of ſupporting the weight not only of a ſuperincumbent ocean, but of all the ſtrata ſubject to that ocean, which by heat are to be converted into ſtone, the air, thus confined in theſe caverns, either would admit of no combuſtion at all, if incapable of expanding, or would ſoon be ſo diminiſhed by the abſorbtion of its vital parts, as to admit the eſcape of fixed air from the ſubſtances acted upon (which would then be calcined) or ſo contaminated by mephitic and fixed airs as to be incapable of producing the violent and extreme effects required by this hypotheſis; then where ſhall we place the ſtrata to be acted on? if over the coaly vaults, they will not be in contact with the flame; if under or on one ſide, only their ſurface can be acted on. Thus, on whatever ſide

we

we contemplate this hypotheſis, it preſents nothing but incompatibilities with our actual knowledge of the operation of fire.

SECONDLY, Our author's demand, that we admit a degree of heat acting with the greateſt poſſible intenſity, and yet producing no change in the ſubſtances acted upon, is not only gratuitous, as he himſelf ſeems to allow, but incompatible with all phyſical inquiry, and a mere *petito principii.* No cauſe can be traced but by its effects, that is, by the changes it has produced; if theſe are ſuppoſed null, all inquiry muſt ceaſe. To avoid this objection, and countenance this ſuppoſition, our author further demands a degree of compreſſion under which the action of heat could produce no change; but this being another new and independent ſuppoſition, ſhould itſelf be proved to be probable, or at leaſt poſſible, which our author neither has done, nor, as far as I can ſee, can do; and even, with the help of this double ſuppoſition, it cannot be proved that pure calcareous earths can at all be melted with or without emitting their fixed air, as this fuſion has never yet been effected either by concentrated ſolar heat, or by the help of pure air, or by any other contrivance of art; and if it could how could the ſhells, with which it is in ſundry inſtances filled, eſcape fuſion, and remain unblended with the common maſs in which they were imbedded?

OUR author will probably reply, that many difficulties alſo accompany the ſuppoſition of an aqueous ſolution, and this I freely confeſs. In the actual conſtitution of things, both phyſical and moral, many inexplicable difficulties occur, but muſt

muſt we not diſtinguiſh thoſe which *eſcape* our reaſon, from thoſe that formally *contradict* it? The former may *in time* be connected with our actual knowledge, the latter never. In our preſent view of nature all appearances point out a watery ſolution, or diffuſion and concretion in that fluid, though the means of effecting this ſolution are but imperfectly known; but they contradict the idea of an igneous ſolution, as we ſhall at preſent prove.

To reduce the perſpective of the mineral kingdom within the bounds of an academical diſſertation, we muſt neceſſarily confine it to the general claſſes under which minerals are commonly arranged, and a few ſpecies of each. And firſt, as to the calcareous claſs. Stones of this claſs, when perfectly pure, or nearly ſo, as ſpars and granular marbles, are abſolutely infuſible in any degree of heat yet known, as Lavoiſier, Geyer, and Ehrman have ſucceſſively ſhewn *. On the other hand, the perfect cryſtallization of the former, and the internal conſtitution of the latter, confeſſedly prove that they were once in a ſtate of perfect ſolution, and ſince they could not be ſo in the igneous, they muſt have been ſo to the aqueous fluid; if we ſuppoſe their particles to have been originally in that ſtate of diviſion which actual ſolution requires, which ſtate may as well be ſuppoſed to have been their primordial ſtate as any other, there will be no difficulty in ſuppoſing them diſſolved or ſuſpended

* Mem. Paris 1783. Schewed. Abhand. 1784. p. 127. Vers. Einer Schmelſkunſt. Von Ehrman.

ſuſpended in an aqueous fluid. As to the compact limeſtones and marbles, in which the teſtaceous exuviæ of marine animals abound, it is evident that if theſe ſtones were ever melted, thoſe would, with them, run into one common maſs, as we have already ſaid. Other ſtones of this claſs are more impure, and mixed with argill and ſilex in ſuch proportion as to be vitrifiable in ſuch heats as art can eaſily produce, yet we never find them in that ſtate; a circumſtance which clearly excludes all ſuſpicion of their ever having been expoſed to them.

In the *muriatic claſs*, we ſee ſteatites and pott-ſtone, which in their actual ſtate have a ſoft ſoapy feel, but harden when heated, vitrify in a ſtronger heat, and acquire a texture and hardneſs quite different from thoſe they before poſſeſſed. Steatites often contain 16 per cent. of air and water; theſe characters depoſe in favour of an aqueous origin: but ſerpentines, of which whole mountains often conſiſt, demand this origin more loudly; for they are infuſible in all but the moſt extreme degrees of heat, in which they vitrify and acquire the poliſh, texture, and luſtre of glaſs.

In the *argillaceous claſs*, we meet with argillaceous ſlates, hornblends, and trapps or baſalts; all of which are in a moderate heat converted into ſlags, whoſe appearances totally differ from that which theſe ſtones preſent in their natural ſtate; and hence they evidently diſclaim an igneous origin. Mica has been clearly proved to originate in water by Mr. Nauovarke, 1 *Chy. An.* 1786.

In

In the *ſiliceous claſs* we have quartz or cryſtal in various regular forms, which, if fuſed at all, muſt have been in the thinneſt fuſion, to be enabled to aſſume thoſe ſhapes. Now the ſtrongeſt heat that art can produce is ſcarcely capable of producing the ſlighteſt emolleſcence in pure quartz; how then can we aſſume that nature, in the moſt unfavourable circumſtances, could produce a perfect fuſion of that ſubſtance? Volcanos afford the moſt intenſe natural heat with which we are acquainted; yet the moſt ſturdy volcaniſts allow it to be infuſible in theſe. In fact it is frequently found in circumſtances in which it is impoſſible, conſiſtently with the known laws of nature, to attribute its origin to igneous liquefaction; for inſtance, it is frequently found cryſtallized in company with calcareous ſpar, fluors, lead ores, &c. on ſtones of a mixed nature, as Petroſilex, Hornblends, &c. Now it is well known that though pure quartz or ſpars will not melt alone, yet in company with ſtones of another kind they will readily melt and unite into one common maſs; when, therefore, they are found in diſtinct maſſes, cloſe by each other, it is evident that they were not formed by fuſion, but in ſome other manner; and there is no other than aqueous ſolution. Of this they bear the marks, for they decrepetate for the moſt part when heated, and become opake from the loſs of their watery particles; though the quantity of theſe involved in their texture be exceeding minute. Have not ſhells and chalk, and even water, been found incloſed in ſilex*? The impreſſion of ſhorls has

* 41 Roz. 34. Mem. Dijon 1783 per Camus.

often been found on the quartz that inveloped it *. This laſt muſt therefore have been in a ſoft ſtate, while the ſhorl was in a hard ſtate; now this could not happen if the quartz were ſoftened by heat, for ſhorl, being infinely more fuſible, muſt have been in a ſoft ſtate alſo, and concrete long after the quartz: nay, if we credit Mr. Gerhard and others, cryſtal has been detected in a ſoft ſtate. We have already quoted Mr. Laſſone as an eye witneſs of the aqueous formation of ſiliceous ſtones. I ſhall only add, that petroſiliceous and other fuſible ſtones of this claſs have quite a different aſpect when they paſs through a ſtate of fuſion from that which they preſent in their natural ſtate. I alſo paſs over the moſſes and other vegetable and animal ſubſtances incloſed in agates, &c. as our author pretends to account for their preſervation in the midſt of the moſt raging heat by virtue of a compreſſion, originating, one knows not how, which prevents their combuſtion or charring.

Let us now examine the principal proofs which our author adduces in ſupport of his ſyſtem; that from the inſolubility of calcareous ſtones we have already obviated.

1ſt, "There are ſpecimens of foſſil wood which bear the "moſt evident marks of having been injected with a flinty "ſubſtance in fuſion. This appears from the wood being pene-"trated partially, ſome parts not having been penetrated at all. "In

* 2 Romé 267 in Note. 1 Chy Ann. 1786. p. 174.

" In the limits of thoſe two parts we have the moſt convincing " proofs that it had been flint in a ſimple fluid ſtate, which had " penetrated the wood, and not in a ſtate of ſolution." Why? " becauſe the flinty ſubſtance has proceeded to a certain length, " and no further; and there is no partial impregnation " nor gradation of the flintifying operation, as muſt have " been the caſe if ſiliceous matter had been depoſited from " a ſolution *." I own I am at a loſs to perceive the force of this argument, and can ſee nothing in it but mere aſſertion.

2dly, " Sulphur is found naturally combined with almoſt " all metallic ſubſtances, which are then ſaid to be mineralized. " Now no perſon, ſkilled in chymiſtry, will pretend to ſay " that may be done by aqueous ſolution. The combination " of iron and ſulphur, for inſtance, may eaſily be performed " by fuſion; but, by aqueous ſolution, this combination is again " reſolved, and forms a vitriol." That metals may combine with ſulphur in the moiſt way is a fact which perhaps was but little known when our author wrote; it is however at preſent ſufficiently eſtabliſhed. Water may be ſtrongly impregnated with hepatic air; the ſulphur is precipitated by almoſt all metals from this water, and in the ſubterraneous meanders where they meet, being protected from acceſs to atmoſpheric air, there is little danger of the converſion of the ſulphurated metals into vitriols. That ſulphurated ores may be formed, without the help of heat, is incontrovertibly proved by their having been

 found

* Page 233, 234.

found overlaying the tools of workmen in old galleries of mines. See the 3d Letter of Baron Trebra, in his Treatiſe on the Internal Structure of Mountains.

3dly, " Several metals have been found native." May not they have been ſo originally?

4thly, " Manganeſe has been found in a reguline ſtate by " Mr. De La Peyrouze, and in ſmall grains, as when produced " by fire." True; but it was mixed with a large quantity of iron, which is often found in that form without any ſuſpicion of fuſion. A fire capable of melting quartz might ſurely produce it in larger maſſes.

5thly, " Spar, quartz, pyrites, and other minerals, are " found variouſly intermixed, cryſtallized upon or near each " other, and adhering to coal, or mixed with bitumen, &c. " circumſtances that cannot be explained in the hypotheſis of " ſolution in the moiſt way." Not exactly, nor with certainty; which is not wonderful: but they are ſtill leſs explicable in the hypotheſis of dry ſolution, as muſt be apparent from what has been already ſaid. How coal, an infuſible ſubſtance, could be ſpread into ſtrata by mere heat, is to me incomprehenſible.

6thly, " Dr. Black found mineral alkali cryſtallized, yet " deſtitute of water of cryſtallization, which could not happen " unleſs it were cryſtallized by fuſion." What then will our author ſay of the vaſt maſſes of this ſalt which are found with their

their full portion of water of cryſtallization? The author refers us to the 71ſt volume of the Philoſophical Tranſactions for an account of Dr. Black's paper. However, in thoſe of the Royal Society of London (the only known by that title without addition) no ſuch paper is to be found. If the alkali were fuſed, the bodies in its neighbourhood were fuſed alſo; without ſome knowledge of their ſtate nothing more can be ſaid; the caſe is not fairly before us. I make no doubt, however, but Dr. Black has examined all circumſtances with that ſkill and accuracy which he is known to poſſeſs.

I DECLINE mentioning a few other diffuſe objections to the aqueous theory, which appear to me to ſhew nothing more than the difficulty of accurately explaining various circumſtances of the mineral kingdom. The only point to be conſidered is which of the two ſyſtems, the aqueous or the igneous, is, upon the whole, leaſt exceptionable, and on this head enough has been already ſaid. I cannot however omit noticing, for the ſake of the diſcuſſion it leads me to, that the application of our author's ſyſtem to the formation of granite is peculiarly unhappy. This rock is formed of ſtones of different degrees of fuſibility, which, in a heat capable of melting quartz, ſhould naturally run into each other; it moſt frequently contains mica, which, when melted, aſſumes an appearance very different from the plated ſtructure it naturally preſents; and, to crown all, can be formed in the *moiſt* way, but cannot in the *dry:* Here I have the misfortune of differing with another zealous patron of the Igneous Theory, equally ſkilled in mineralogy and chymiſtry, the

the learned Dr. Beddoes. In the Philoſophical Tranſactions for 1791, *Page* 56, &c. he tells us, that "a mixture of different "earths, with more or leſs metallic matter, in returning from "a ſtate of fuſion to a ſolid conſiſtence, may aſſume, ſometimes, "the homogeneous baſaltic, and ſometimes the heterogeneous "granitic internal ſtructure. No fact is more familiar than "that it depends altogether on the management of the fire, "and the time of cooling, whether a maſs ſhall have the uniform "vitreous fracture, or an earthy broken grain ariſing from "a confuſed cryſtallization. The art of making Reaumur's "porcelain conſiſts entirely in allowing the black glaſs time to "cryſtallize by a ſlow refrigeration, and the very ſame maſs, ac-"cording as the heat is conducted, may without any alteration of "its chymical conſtitution be ſucceſſively exhibited any number "of times, as glaſs, or as ſtony matter with a broken grain. "In the ſlagg of iron furnaces the ſame pieces generally ex-"hibits both theſe appearances." How far the ſame maſs in fuſion is capable of aſſuming ſometimes, the baſaltic and ſometimes the granitic, we ſhall preſently ſee. With reſpect to Reaumur's porcelain, it is certain that the changes of texture, mentioned by the learned author, may be produced in it, not by a ſlower or more rapid cryſtallization (for in fact there is no cryſtallization at all) but by the continuance of a higher or lower degree of heat. This is evident from the experiments of Dr. Lewis and Mr. Delaval. Now the effect of the higheſt heat of our furnaces, in this caſe, is to rob the glaſs of its ſaline part, as Dr. Lewis well obſerved; and hence it is not wonderful that the texture ſhould be altered, and the maſs at laſt become

looſe

looſe and porous. It cannot, therefore, be ſaid that it retains the ſame chymical conſtitution as before; the caſe is quite different with reſpect to glaſſes formed of earthy ſubſtances without any ſalt, as I know from my own experience, when once they are perfectly vitrified, a ſecond fuſion makes no alteration whatſoever in them, though ever ſo ſlowly cooled. Thus, feltſpars, garnets, ſhorls and baſalts, being converted into glaſs by the heat of a furnace, remain glaſs even when expoſed to the higheſt heat producible by art, namely, that ariſing from the action of pure air; nor will any retardation of their cooling produce the ſmalleſt change. As to the ſlagg of iron furnaces, it is a compound in which the metallic particles, being by far the moſt abundant, ſeparate themſelves, during fuſion, from the earthy. Theſe laſt then vitrify, vitrification being the effect of the heat to which they are then expoſed, and not in conſequence of their rapid refrigeration; the metallic particles, on the contrary, aſſume the grain that is peculiar to them, being incapable of vitrification; hence all analogy with baſalt fails.

THE Doctor, however ſenſible of the difficulty of ſuppoſing that a ſubſtance once uniformly fuſed, as he imagines granite to have been, ſhould preſent us 2, 3, 4, 5 and 6 ſeparate ſubſtances, as granites frequently do, further adds, "That "this difficulty does not preſs the igneous more than the oppoſite "hypotheſis, ſince the conſtituent parts of granite are cryſtals, "the whole maſs muſt have once exiſted in that ſtate of entire "diſunion of its particles which is neceſſary to cryſtallization.

Now

" Now whether ſuch a ſolution has been effected by the repulſive " force of fire, or the intervention of water, it is juſt as eaſy " to conceive heterogeneous earthy cryſtals, ſhooting from " different points of an uniform liquid, according to the former " ſuppoſition as the latter." It is true, by abſtract conſiderations, we may conceive any thing; but to form juſt conceptions of the operations of nature we muſt take *experience*, or, where this fails us, *analogy*, for our guides. Here both lead us to conceptions diſagreeing totally from the Doctor's. Experience tells us that granites, once perfectly fuſed, coaleſce in cooling into a *greeniſh white* or other *coloured glaſs* *, ſo different from baſalt, that the experimenter, from this experiment alone, was tempted to conclude that baſalt muſt have been produced in the moiſt way. Analogy ſuggeſts that as ſalts of different degrees of ſolubility, in a liquid menſtruum, being brought to cryſtallize, cryſtallize ſeparately, but if fuſed in fire never can; ſo ſtones of different degrees of ſolubility in a liquid menſtruum, being brought to cryſtallize, ſhould cryſtallize in ſeparate concretions. Even a *priori* cryſtallization into ſeparate heterogeneous maſſes is much more eaſily conceived in an aqueous than in the igneous fluid. This laſt occupies no perceptible ſpace, and all the particles it holds in ſolution are on that account crouded together, and in full contact with each other; in proportion as the igneous fluid decreaſes they loſe that facility of motion that is neceſſary for the union of the homogeneous parts and regularity of arrange-

* Per Hacquet 1 Crell. Beytr. 35, & Morveau in 1 Buff. Mineralogy p. 139, in 8vo.

arrangement; ſo that ſcarce any thing but a difference of ſpecific gravity can, while they are in full fuſion, produce a ſeparation. While cooling, ſuch a ſeparation cannot poſſibly take place, according to our conceptions. On the other hand, if ſtony maſſes be once conceived diſſolved in water, this fluid, occupying a much greater ſpace, will allow them full room to concrete in ſeparate maſſes, according to the laws of their various affinities; and this is ſo true, that if evaporation be carried too far, they cannot be properly ſeparated by cryſtallization.

To cloſe this controverſy, I ſhall only add, that granite, recently formed in the moiſt way, has frequently been found; but no inſtance can be produced of its formation by fire. Thus a mole, having been conſtructed in the Oder in the year 1722, 350 feet long, 54 feet in height, 144 feet broad at bottom, and 54 feet at the top, its ſides only were granite, without any other cement than moſs; the middle ſpace was entirely filled with granite ſand. In a ſhort time this concreted into a ſubſtance ſo compact as to be impenetrable by water *. Mr. Soulavie diſcovered an enormous fiſſure in a marble rock, filled with granite matter, which muſt have been in a liquid ſtate when the marble was already ſolid; elſe it would have mixed with it, and not have filled, as it was found to do, all the ſinuoſities of the calcareous rock †.

* Laſius Hartz.

† 1 Soulavie France Merid. 385.